during warm sunny days in winter, and again disappearing on the return of cold. This fact it is certainly very difficult to account for on any other supposition. ARGYLL

Argyll Lodge, Kensington, April 11

"In your letter received last night you tell me of an article in NATURE the author of which seems to deny that swallows ever hibernate, and asserts that no one has yet testified to the fact from his own personal observation. That, however, is a mistake, for I have stated and I now repeat that I have seen swallows in large numbers hibernating. The circumstances were these:—

"About twenty-five miles south of Teheran, the capital of Persia, there is a village called Kenara-gird near which is a stream of brackish water running in a deep bed with nearly perpendicular banks some forty or fifty feet high. Being largely impregnated with salt this stream is rarely if ever frozen, and in frosty weather is resorted to by flights of wild ducks. During a frost of unusual severity I went from Teheran to Kenara-gird, accompanied by Sir Henry Rawlinson, for the purpose of duck-shooting, the severity of the frost promising good sport. Having slept at the village we next morning followed the downward course of the stream along the north bank, and had proceeded about a mile, I should think, when we came to a place where there had quite recently been a small land-slip. The brink of the bank to the extent of perhaps twenty feet in length, and ten or twelve broad in the middle, tapering off to each end, had slipped, but had not fallen down the bank. Between this detached portion and the perpendicular face about ten feet high, from which it had broken off, we saw, to our great surprise, a number of swallows, not less, I am sure, than twenty or thirty, lying, as I at first supposed, dead, but on taking up one of them I found that it was alive but dormant; it was warm and its breathing was quite perceptible. I examined a considerable number, and found that they were all alive and breathing, but none of them gave any sign of consciousness. My attention was then attracted to the perpendicular face on our left, from which the slip had broken off, and which was perforated by a vast number of holes each about the size of a rat-hole. On looking into such of these as I was tall enough to see into, I found in all of them swallows in the same dormant state. I was able with finger and thumb to pull out swallows from several of these holes, and in each case found that the hole which penetrated horizontally a considerable way into the bank, contained more swallows in the same condition. In no case did I see one lying on another—they were all lying singly with their heads inwards, each head touching the tail of the bird before it. How far these holes penetrated into the bank, or what number of swallows each contained I did not ascertain, but it is plain that the original entrance to these dormitories, must have been in the external face of the portion that had slipped, which as I have stated, was, in the middle, from ten to twelve feet thick. The holes in the undisturbed portion may probably have been of equal or greater length, and if so the number of swallows hibernating there must have amounted to many hundreds."

Villa Poralto, Cannes, April 6

The Swallows and Cuckoo at Menton

THE swallow that arrived here on March 19 remained solitary until April 5. Early that morning a second arrived and entered the same room as the first. I saw them flying about together in the forenoon, and these two remain the only feathered occupants of that chamber. In the afternoon of the same day, however, a party of ten arrived and distributed themselves among the houses.

Madame Valetta, of whom I spoke in my previous letter (NATURE, vol. xv., p. 488), assures me that not during the last fifty years has one swallow preceded its fellows by so long an interval as this year; but perhaps it is only that her attention has not been drawn so much to the subject. It is certain, however, that unless more are yet to come, the swallows are this year fewer than ordinary by more than one-half. The opinion of the natives is that they have perished at sea.

I heard the cuckoo for the first time this year on April 1. But a lady had told me that she had heard it nearly a week before. DOUGLAS A. SPALDING

Cabrolles, près de Menton, France, April 13

Greenwich as a Meteorological Observatory

THE facts of observation appealed to by Mr. Eaton with the view of proving that Greenwich is, from artificial causes, more than half a degree warmer than the south-east of England generally, and is therefore not a suitable place for a meteorological observatory of the first order, evidently call for a closer critical examination than they have yet received. If from the sixteen stations within a radius of sixty miles from the metropolis given in the paper on "The Temperature of the British Islands," we omit those which are clearly inadmissible for the comparison with Greenwich owing to their position or to the short time during which observations were made, there remain the following eight stations as suitable for comparison:—Cardington, 52° 7′ lat. N.; Royston, 52° 2′; Colchester, 51° 53′; Hartwell, 51° 49; Oxford, 51° 46′; Great Berkhamstead, 51° 45′; Chatham, 51° 23′; and Aldershot, 51° 15′. The mean temperature of these eight stations, allowing for elevation, is 50°·6, and the mean temperature of Greenwich, 51°·1, results substantially agreeing with those given by Mr. Eaton.

It is necessary, however, to observe that the mean position of these places does not agree with that of Greenwich. Thus while the latitude of Greenwich is 51° 28′ north, the mean latitude of the eight stations is 51° 45′, or 0° 17′ farther north, their true mean position being about two miles due north of St. Albans. For this difference in latitude a correction of fully 0°·2 is required, judging from the position of the isothermal lines. The figures then stand thus: mean temperature of Greenwich, 51°·1; of the eight stations, 50°·8. On comparing the monthly means of the eight stations with those of Greenwich, it is seen that the residual excess of Greenwich is all but wholly occasioned by the high mean temperatures of June, July, August, and September, which are in each case 0°·9 higher than the means of the eight stations, these eight stations being nearly all outside or on the outskirts of the patch of high temperature around London during the summer months. It follows that the 0°·3 of excess of annual temperature at Greenwich over the eight stations is accounted for by the higher temperature of four out of the twelve months, and consequently cannot be due to artificial sources of heat in London, such as the consumption of fuel.

When drawing the isothermals of the British Islands seven years ago, no district of Great Britain occasioned so much trouble and uncertainty as the south-east of England, owing to the meagreness of the materials available for the purpose. Since, however, immediately to westward, the mean temperature of Oxford was seen to be 50°·4, Aldershot 51°·2, and Osborne 51°·8, it was inferred as the most probable state of the case that the mean temperature increased southwards over the south-east of England in the manner indicated by the annual isothermals accompanying the paper. This supposition was confirmed by the temperatures at Colchester and Chatham, the only two stations furnishing satisfactory data on the point, for while at Colchester, for instance, the mean temperature was 0°·5 lower than that of Greenwich, no less than 0°·4 of this difference was due to the lower temperature of the east coast at Colchester as compared with Greenwich during the five months beginning with December, when the temperature in the south of England increases from east to west.

The coast stations of the Channel could not be considered as furnishing authoritative evidence on the question, owing to the irregular distribution of their temperature, which seemed to indicate certain obscure and ill-understood causes in operation modifying the climates of that coast. The force of this remark will be apparent from the following mean temperatures:—Helston, 53°·9; Truro, 52°·2; Torquay, 51°·6; Sidmouth, 51°·1; Bournemouth, 51°·5; Ventnor, 52°·4; and Worthing and Eastbourne, both 50°·7. It is perhaps scarcely necessary to remark that it would be a mistake to attempt to draw any conclusion from differences of mean temperatures of different stations amounting to 0°·3 and under, seeing that the English observations generally were made with thermometers in protecting-boxes quite open on one side, and therefore exposed in varying degrees to indirect radiation from walls and other objects.

While Mr. Eaton has thus failed to prove from past observations that the consumption of fuel and the massing together of living beings in London has raised the mean temperature at Greenwich Observatory to the extent of half a degree, or indeed to any appreciable extent, above that of the south-east of England generally, it might nevertheless be well, seeing the question has been raised, to do something towards definitely answering it, by instituting at Greenwich and at about a dozen stations distributed over the south-east of England, observations of the temperature of the air, strict uniformity being secured by employing the same pattern of thermometer-box and by placing it under the same conditions at each place as regards height above the

© 1877 Nature Publishing Group

ground, the vegetable covering of the soil, and openness of situation. ALEXANDER BUCHAN

Edinburgh, April 9

THERE is one consideration which your correspondents, the secretary of the Scottish, and the president of the British Meteorological Societies have equally overlooked, and which may seriously affect the conclusions at which they arrive as to the suitability of Greenwich for a first-class meteorological station. Since the year 1846 the temperature observations at Greenwich have been made under conditions of exposure of thermometers which, whatever their merits or demerits, are not those usually adopted. In a paper published in the *Quarterly Journal of the Meteorological Society* (October, 1873) I have shown from the average of five years' daily comparisons that the effect of the method adopted at Greenwich upon the mean annual temperature is to obtain a result 0°·475 warmer than is obtained by the usual method. This quantity is almost identical with the excess which Mr. Eaton attributes to the local consumption of fuel, an explanation surely most inadequate. Thus the discrepancy pointed out by Mr. Eaton only serves to establish his opponent's case. Mr. Buchan on the other hand must be unaware that it is the eye observations alone, made from the revolving stand, that are relied upon for temperature results at Greenwich, and though his conclusion would seem to be correct it does not seem possible to sustain the argument by which he has arrived at it.

Orwell Dene, Nacton JOHN I. PLUMMER

Cast-Iron

I HAVE been struck by the statement I found in several books on physics that cast-iron expands when it gets cool. As some of these books are used as text-books in schools in this country, and as this statement is contrary to the experience of all practical men with whom I have conversed upon the subject, I think the following translation of an article which appeared in *Der Civil Ingenieur*, edited by K. R. Bornemann, in Freiberg, 1863, ix. Band, iv. Heft, p. 219, may not be uninteresting, explaining, as it seems to me fully, at least one of the facts on which the statement mentioned above appears to be based, viz., the fact that cold iron swims in liquid iron :— H. M.

"At a meeting of the Association of Saxon Engineers which took place in Freiberg in August last year (1862) Mr. H. Gruson, of Buckau, near Magdeburg, called the attention of the members to a phenomenon which had frequently been observed by him, but of which no proper explanation could be given at the time, viz., that pieces of cold cast-iron swim perfectly in molten iron. The question was raised, to what causes this may be due, and as from the physical point of view it was thought an interesting one, it was suggested that experiments should be made in order to obtain a proper explanation thereof.

"In consequence of this, M. Centner, Inspector of the Jacobi Iron-works, near Meisen, made such experiments, the result of which he communicated to the Association at a meeting on May 17, 1863. The following is an extract of Mr. Centner's report :— (Signed) W. TAUBERTH"

"Before anwering this question I made some experiments in order to ascertain whether this swimming is not caused by the specific weight of the body ; by these I found confirmed that cold cast-iron weighs $\frac{1}{25}$ more than an equal volume of molten iron, for if a piece of cold cast-iron of 28 lbs. be used to form a mould, and if this mould be filled with molten iron, the new piece of metal thus obtained will only weigh 27 lbs. This weight, of course, is also that of the liquid metal which was required to fill the mould, formed from a piece weighing 28 lbs.

"For this reason, in making moulds for cast-iron, a measure is used which is $\frac{1}{96}$ longer than the ordinary measure, if the piece of iron to be formed is to have the full size of the ordinary measure.

"Repeated experiments with a mass of molten iron of 2,000 lbs. gave me further proofs that the causes of this swimming must be other than the specific weight.

"For my experiments I used four bodies of cast-iron of different shapes, but of the same volume, viz. a plate of 6″ inches in the square and 1″ thick, a cube of 3⅓″, a cylinder of 4″ diameter, and 3″ height, and a ball of 4″ diameter. Each of these four bodies measured 36″ cube and weighed 7 lbs.

"If the cause of the swimming were the specific weight, an equal part of the volume of all these bodies ought to remain above the surface of the liquid iron, but such is not the case at all. The volume above the surface of the liquid iron is different in each of the four bodies ; it is greatest with the plate and smallest with the ball. Thus it is dependent on the shape and position of the surface which rests on the liquid iron.

"In order to come to the real causes of this swimming, I must first remind the reader that in every hot liquid in an open vessel, in consequence of the more rapid cooling at the surface, a continuous current is originated, the interior hottest parts ascending and the exterior colder ones descending ; and thus a more or less visible movement or agitation is produced in the mass. Such currents occur in every mass of molten iron, and are there especially remarkable in consequence of a contemporaneous ascension and separation of slags, which, when they have arrived at the surface, are generally pushed towards the edges.

"If a solid piece of iron be put on the liquid mass the former gets at once heated at the expense of the latter ; the portions of water and of air which are contained in the solid piece get expanded and expelled with considerable force, thus forming a current in opposition to the ascending one above mentioned.

"This expulsion of air and water may even cause dangerous explosions, if the usual precaution is neglected to warm the solid piece somewhat, before it is brought into contact with the liquid mass.

"Now there is no doubt that these opposite forces alone are able to raise the heavier solid piece more or less according to the more or less favourable surface it presents.

"But besides this there is to be taken into consideration that the overweight of the solid piece of iron is diminished by the previous heating which when the solid piece comes into contact with the liquid, is at once augmented and that the proportion of heat of the molten iron to that of the solid piece must to some extent have an influence on the more or less deep immersion of the latter.

"A further cause, although a slight one, of this swimming of the solid piece is the cohesion of the liquid iron ; but at any rate this becomes of some importance in conjunction with the above-mentioned continuous ascending of slags which collect under the swimming body and retain partly the air expelled by the latter, helping in this way to keep it afloat.

"Solid cast-iron being $\frac{1}{28}$ heavier than an equal volume of molten iron, the overweight of each of the four bodies used for my experiments is consequently only ¼ lb., and in the present case it is therefore only this one *quarter of a pound* which the above-mentioned opposite forces have to lift within areas of 12″ to 36″ square in order to keep the body swimming.

"On increasing the size of the solid bodies, however, it will be easily understood that a limit to this swimming will soon be reached, and indeed I accidentally found this limit on my first trial in quadrupling the sizes of the four bodies so that each of them weighed 28 lbs. ; for all the bodies with the exception of the plate which was 12″ in the square and 1″ thick went to the bottom. The plate on being put gently on the liquid mass was just kept afloat, but its surface was a little below the surface of the molten iron. 1 lb. overweight therefore with a surface of 12″ in the square could scarcely be kept swimming.

"The behaviour of the other three bodies at the bottom was remarkable in consequence of a continuous vehement ebullition accompanied by the shooting out of long white brilliant flames, and these phenomena can only be attributed to the water and air expelled by the heat.

"These experiments with bodies of 28 lbs. weight show therefore that above this weight, without giving to the body a more favourable surface than 12″ in the square, these bodies do not what is properly called swim in the sense that part of the solid body is kept above the level of the liquid. For if the bodies, for instance the ball, the cube, and the cylinder on being moved, rise and fall a little alternately, this can no longer be called swimming, for it is just the transition from overweight to equilibrium.

"That the greater or lesser degree of density of the different sorts of iron will also exercise an influence cannot be doubted.

"Less fortunate but still interesting was an experiment which I made with four pieces of zinc of the same shapes as the pieces of iron on 200 lbs. of liquid zinc, when with the pieces of 7 lbs. each the same thing took place as with the cast-iron pieces of 28 lbs. each on liquid iron, viz., the plate was just kept afloat and the three other bodies went to the bottom.

"With zinc, therefore, this phenomenon of swimming does not occur with such heavy bodies as with iron, and this may be explained by the fact that with zinc, in consequence of the much smaller difference of temperature between the liquid and the

© 1877 Nature Publishing Group